

June 17, 1950.

Dr. M. R. Zelle,
30 Decatur Street,
Kensington, Md.

Dear Max:

The story on your series of 6/5 is as follows:

Except for M, all initials were typical H226 diploid. All isolates were still type diploid except H-12.mmx (Very low segregation frequency!) H-12 is haploid, Lac- Mal+ Xyl+ Mtl+ auxotroph.

M is peculiar! Descendants of M-1 are partial segregants, i.e., Lac- Malv Xylv MtlV. M-2 descendants are haploid segregants, Lac- Mal+ Xyl+ Mtl+. I have found quite a few such partial segregants in H-226 cultures, and a great many after irradiation. I have been working on the assumption that they represent cases of refusion after meiosis, a sort of autogamy, which can result in homozygosis of some factors, if crossing over has occurred. For example, we might have:

Reversion tests on such partial segregants (Mal-Lacv or MalV Lac-) have indicated that they are homozygous, rather than hemizygous, for "-".

Unfortunately, this segregation gives no special information, since it may be assumed that M itself was like M-2, and that M-1 is a segregant from it. Too bad we haven't found an instance of partial segregation itself, but it is much rarer than segregation to haploids.

K-3 was missing.

We are leaving in a couple of days for Berkeley, where I'll be teaching during August. Until about August 1, you can reach me most expeditiously by addressing me here, and marking the envelope "URGENT". After August 1, I'll be at the Bact. Dept. I hope to see you at Columbus this September.

If you're anxious to do any more single cell isolations this summer, I can have one of my assistants carry on- he's been doing most of it anyhow, and has a pretty good idea of what's what. For this purpose, mark the packages "c/o N.Z." I wonder if ~~HXX~~ H226 isn't stable enough to stand shipment by parcel post. They have stood up quite well several days after receipt here.

Do you think you'll have a chance to work on the MS?

Sundry people here have been asking me if I knew a fellow named "Beckhorn". I have the impression he may be your student, Can you tell me anything about him? I don't think I've seen any publications, but I have had a request post-card from him for every reprint.

WE'll be in Madison till next Friday, probably, (June 23) so there might be time for a quick answer or other discussion. My next definite stop will be Denver (Biophysics Dept., Medical School, c/o T.T. Puck) probably about the 30th.

The radiation story has gotten very complex, and will have to be tied in with a detailed cytological analysis. Haploidization is only a small part of the picture. The surviving diploids are messed up in a lot of ways. About the only thing that can be said is that lethal recessive mutations play a negligible ~~xx~~ role in killing multinucleate or diploid cells. We had a conference here last week, and Atwood and Latarjet have come to the same conclusion with Neurospora and yeast respectively. Lethal recessives probably do play some role in haploids, but this is very difficult to verify accurately.

Sincerely,

Joshua Lederberg
Associate Professor of Genetics